LETTERS TO THE EDITOR.

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Measurements of Crabs.

On June 11 last, a paper by Mr. Herbert Thomson was communicated to the Royal Society, "On certain changes observed in the dimensions of parts of the carapace of *Carcinus mœnas*" (*Proc. Roy. Soc.*, lx. No. 361). According to measurements recorded in this paper, the male crabs taken at Plymouth in the year 1895 were narrower in the frontal breadth, and longer in the right dentary margin than male crabs of the same size taken in 1893.

The author of the paper states that further measurements will be necessary in order to decide whether these results indicate a permanent change in the species at Plymouth, or a mere oscillation, such as may be constantly going on in the relative dimensions of parts in a species.

I venture to remark that species must be much more unstable than we have ever supposed hitherto, if either permanent changes or mere oscillations in their characters are to be detected in one locality in the course of a couple of years. There is one other possible explanation of the observed differences which has occurred to my mind, and is, I think, worth consideration on the part of those who are studying evolution by means of the micrometer. It appears from the paper that the measurements of the 1893 crabs and of the 1895 crabs were made, not in those years respectively, but both sets alike after the summer of 1895. The two sets of crabs were, in this case, not measured under identical conditions. If the 1893 set had been longer in spirit than those of 1895, perhaps this was the reason of the difference. Or there may have been some difference in the mode of preservation. At any rate, I think the comparison is not trustworthy unless the measurements had been made on fresh crabs immediately after they were collected—one set in 1893, the other set in 1895. It is doubtful whether measurements of spirit specimens are ever perfectly trustworthy as representing the true dimensions of animals.

The fact that a deficiency in one dimension was "compensated," as the author himself expresses it, by an excess in the other, suggests the question whether the specimen of one lot or the other had not undergone an artificial change of shape. It seems to me that that question must be disposed of before we admit that a permanent or temporary change in the specific dimensions of parts in the crab has been demonstrated.

J. T. Cunningham.

Some Effects of the X-Rays on the Hands.

At the request of the editor of Nature, I append the following description, compiled from notes, of the effect of repeated exposure of the hands to the X-rays. The result, though perhaps interesting from a medical and scientific point of view, has been most unpleasant and inconvenient to myself—the patient—and although my theories may be incorrect, and my conclusions easy to demolish, there is no mistaking the fact that the X-rays are quite capable of inflicting such injury upon the hands as to render them almost useless for a time, and to leave in doubt their ultimate condition when entirely freed from frequent daily exposure to their influence.

Now for facts. I commenced demonstrating early in May with a coil capable of giving an 8" spark, and have been engaged in the work for several hours per day until the present time. For the first two or three weeks no inconvenience or discomfort were felt, but there shortly appeared on my right-hand fingers numerous little blisters of a dark colour under the skin. These gradually became very irritating, the skin itself very red and apparently much inflamed. The irritation increased, and the application of *aqua-plumbi*, as recommended in a Berlin telegram to the *Standard*, had only a passing effect in allaying it. So badly did my hand smart, that I was constantly obliged to bathe it in the coldest water I could get, and I really believe I should have been obliged to resign my appointment had not a well-known medical man, who happened to attend one of the demonstrations, advised me to use a much-advertised ointment. I did so, with the remarkable result that the irritation left me immediately, and by using it regularly since then, I have at least avoided one of the disagreeable consequences of too much X-rays. In the meantime, however, the skin on the fingers had become very dry and hard, yellow like parchment, and quite insensible to touch, and I was not at all surprised to find, a day or two afterwards, that it began to peel off. When this particularly unpleasant operation had been accomplished, I considered I was quite acclimatised to the rays, but soon found out my mistake. The same symptoms again appeared, the newly-formed skin going the same way as in the former case. But there was a further discomfort to follow. About the middle of July the tips of my fingers began to swell considerably, and appeared as if they would burst. The tension of the skin was very great, and, to crown all, I noticed for the first time that my nails were beginning to be affected. This was the commencement of a long period of really serious discomfort and pain, which was only partly relieved when, from under the nails, there appeared a somewhat copious and unpleasant-smelling colourless discharge, which continued more or less until the old nails were thrown off. With this discharge the swelling in the finger-tips decreased, but as the new and old nails began to separate in the middle, the pain was renewed, and I was unable to bear the slightest pressure upon them. The old nails turned quite black and very hard, and the state of my hands may be imagined when I say that I had to keep the fingers in bandages for more than six weeks. It was only in the middle of August that my left hand became affected by the rays, as until then I had principally used my right hand in the manipulation of the fluorescent screen. I naturally expected to again undergo the same experience, with all its discomforts. I had lost the skin of my right hand for the third time, and there seemed to be no probability of that being the last. Several doctors had seen my hands, and taken much interest in their condition, but no one could suggest a remedy.

At last it occurred to me that all the trouble was being caused by the rays burning out the natural oil of the skin, and that if I could in some way supply the deficiency, it might assist in preventing further ill effects. For that purpose I got some lanoline, the oil obtained, so I am informed, from sheep's wool. This I daily rubbed into my hands, and then encased them in a pair of ordinary kid gloves. These gloves, in the course of time, became saturated with the ointment, and there is no doubt that, although in themselves they were quite transparent to the X-rays, and therefore no shield in themselves, the fatty matter did, in a great degree, prevent the drying up of the skin in the manner I have described. I do not mean to say that it is an absolute preventive, but it goes a long way towards that desirable end, because since I first used the lanoline, now some weeks since, my hands have not again peeled, although at the present moment (October 17) there are a few slight symptoms of it.

My view of the effect of the X-rays is that, in regard to this matter, it is exactly similar to acute sunburn. The symptoms and effect are the same, only that, in the case of the X-rays, you have it in a far more concentrated form—in fact the very essence of it. But whatever may be the cause, the effect is unquestionable. In my case I have had three new sets of skin on the right hand, and one on the left; four of my finger-nails have disappeared on the right, two on the left, and three more are on the point of leaving. For at least six weeks I was unable to use my right-hand fingers in any way whatever, and it is only since the nails came off that I have been able to hold a pen. Of course it will be a month or two before my hands resume their natural condition, and it is yet, as I said before, a moot point as to what the end will be.

I could say much more on this subject, but already I fear I have trespassed too much on the editor's space. I have written this with the object of placing upon record "the strange case of an X-rays operator," in the hope that it may add something to what is known of the new and mysterious power, and lead others, more experienced in scientific and medical knowledge than myself, to devise an effectual preventive against such results as I have described. Many important questions are opened up by this remarkable effect of the rays upon the skin and nails, and it may be that in the near future they may be utilised in cases of skin and other diseases. Who knows? S. J. R.

X-Rays Syndicate, Indian Exhibition, Earl's Court, London.

Habits of Chameleons.

I have just read Mr. Ridsdale's letter about the Chameleon, and write to say that I have one here which has lived in England since May 23, 1891, when it was brought from the Cape by my

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nephew. I imagine it must be somewhat different to Mr. Ridsdale's specimen, judging from the variations of colour. This one has green as its predominant colour, changing in a bright light to a brown or almost chocolate hue, and at night it is often a bright canary yellow, especially when kept, as it was on its first arrival, in a cage which was painted cream colour inside. Only once have I seen it turn white, and that was when I was just in time to save it from being killed by a cat, and then I suppose it was the result of fear. It frequently has yellowish stripes running along its body, and sometimes round red spots. Not unfrequently one side of its body is of a different colour to the other. It drinks only sparingly, but I saw it do so this morning, putting its head right into the glass with which it is supplied.

After it had been in England about a year it surprised us by laying some eggs, and has done so again within the last few months. The eggs are roundish, about the size of small peas, and of a clear orange colour, somewhat resembling grains of maize. If either Mr. Ridsdale or Mr. Bartlett would like to have one, I should be pleased to send them specimens.

The last time it changed its skin it had the misfortune to lose about half an inch of its tail, from what cause I am unable to say, and this has made it much less able to get about, as the loss deprived it of the little hook at the end of the tail with which it used to cling to the sticks on which it climbs. I never saw it try to help the skin off with its feet, and it has generally come off in flakes, taking a fortnight or so over it.

I think the most extraordinary thing about the reptile is the wonderful way in which the two eyes work quite independently of each other, and enable it to survey comfortably objects in quite opposite directions. A. ALEX. BLAKISTON.

Glastonbury, October 21.

Chameleons at the Zoological Society's Gardens.

MR. RIDSDALE must have mistaken Mr. Bartlett when he states (NATURE, October 15) that there are no Chameleons in the Zoological Society's Gardens. We are seldom, if ever, without examples of these reptiles, and at the present moment have five specimens, three of *Chameleo vulgaris* from North Africa, and two of *Chameleo pumilus* from the Cape. At various times we have exhibited specimens of eight species of Chameleonidæ. I may add that Chameleons generally do not do well in captivity, and require constant attention. P. L. SCLATER.

3 Hanover Square, London, W., October 22.

The Organisation of Technical Literature.

THE "Catalogue of Scientific Papers," compiled and published by the Royal Society of London, was intended to serve as an index to the titles and dates of scientific papers contained in the Transactions of societies, journals, and other periodical works. This Catalogue is highly valuable to all technical inquirers, and it is a matter of deep regret that the International Conference, held under the auspices of the Royal Society of London, has decided that the International Catalogue of Scientific Literature, which is to begin with 1900, is to relate to pure science only, applied science being strictly excluded. It is possibly too late to remedy the position, which is probably due to the absence of representatives of technical societies at the International Conference.

It would seem desirable, further, that there should be a conference of technical societies to discuss the publication of a subject-matter index to technical and scientific periodicals. The Federated Institution of mining engineers has had for some time before it the question of the publication of such an index of subjects of interest to mining and metallurgical engineers; and probably a comprehensive index to engineering and other technical papers would prove more valuable.

This suggested conference of technical societies might also consider other questions which interest technical societies individually, but which they are unable to obtain owing to want of concerted action. Thus, such an association might approach the Government on such questions as the excessive cost of postage of Transactions, as there can be no valid reason why they should not be placed in the same position—although their Transactions are issued at varying intervals of time—by a short Act of Parliament, as an ordinary weekly newspaper.

And there are many other matters, which no doubt crop up in connection with the carrying out of the objects of individual societies, in which concerted action would produce valuable results. M. WALTON BROWN, *Secretary*.

Neville Hall, Newcastle-upon-Tyne, October 21.

A Mechanical Problem.

A MAN stands in a box, whose sides, floor, and top are rigid and inelastic, the box itself being light as compared with the weight of the man, and a few inches higher than himself. The man jumps, and strikes the roof with his hands or head.

Is it possible for him to raise the box in this way (even in a small degree) perpendicularly from the ground? "CROMERITE."

[(1) The box rests on the ground. Therefore the downward push of the man on the box on springing is balanced, and no more affects the motion of the box than if his feet rested on the ground. If his muscular force were great enough, he could give infinite velocity to his body. He is now moving, the box still at rest as before. When his head strikes the top, the rate of destruction of his upward momentum is the upward force on the box, and this depends on the elasticity of his head. If his head is rigid enough he lifts the box, however heavy it may be.

(2) The "argument waged round" the question is possibly based on the idea that the box and man are in free space. In free space, nothing that the man can do will affect the motion of the centre of mass of the whole system (the box-man system); but the box itself moves, so that even in free space "the man moves the box."—J. P.]

Extension of the Visible Spectrum.

WHILE engaged on work in connection with the discharge of electrification by ultra-violet light, we have come across a fact which it may be convenient to state by itself, viz. that the spectrum of an arc can be made visible over the greater part of its immense range of action on electrified metals, by receiving it upon a screen of the double fluoride of uranium and ammonium, such as is frequently used for displaying X-rays. The arc-light must, of course, be passed through a quartz train, and a long arc is best, especially an arc containing aluminium; but under these circumstances, whereas the ordinary visible spectrum may be three-quarters of an inch broad, a breadth which may be doubled or trebled by the use of ordinary fluorescent substances, the spectrum received upon a uranium screen is five inches broad, and is full of bright lines.

Suitable screens are supplied by Ducretet of Paris, or by Chadwick of Manchester, or they can be readily made; and any one possessing either a Rowland grating, or a quarter prism and lens, can try the experiment.

It is just possible that it has not been noticed, to the full extent, before. OLIVER J. LODGE.

Liverpool, October 26. BENJAMIN DAVIES.

ON THE COMMUNICATION OF ELECTRICITY FROM ELECTRIFIED STEAM TO AIR.[1]

THE experiment described in this paper was a first slight instalment of an investigation, which we have proposed to make, of the diffusion of electricity through air, and the communication of electricity from the molecules of one gas to the molecules of another beside it or mixed with it.

By arrangements, readily imagined, we electrified dry superheated steam at atmospheric pressure by a needle-point connected with an electric machine. The dry electrified steam was drawn off through a tube with an inlet admitting unelectrified air to mix with the steam. The mixed air and steam were drawn off through the metal worm of a still, and cooled by an abundance of cold water around the worm. The condensed water fell into a Wolff's bottle, in one neck of which the exit tube of the worm was fixed. The air, thus cooled and partially dried, was drawn out of the other neck of the Wolff's bottle through a drying tube of pumice and sulphuric acid; and thence through a short paraffin tunnel to one of our electric filters,[2] insulated and connected with the

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[1] Abstract of communication to Section A of the British Association at Liverpool (September 21), by Lord Kelvin, G.C.V.O., F.R.S., Magnus Maclean, and Alexander Galt.

[2] "On the Diselectrification of Air," *Proc. R.S.*, March 1875.

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